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# IS THE CONSERVATION OF ENERGY PROVED OF THE HUMAN BODY?

THE philosopher, learning nature's laws at second hand from the scientist, labors under a disadvantage. He is accustomed to make sweeping statements; his discipline approves nothing short of universal judgments such as "consciousness is coordinated behavior" "all events are caused," *etc.* Accordingly when he finds, or thinks that he finds, in certain of the sciences some very widely attested law, or some all but universal habit of mind or of method, he leaps to an absolute principle and attributes to the law, the habit, or the method, an authority brooking no exceptions. He forgets that in the history of science results apparently final have been superseded. Chemical atoms have been analyzed; gravitation may become a residual electrical phenomenon; the ideal of mechanical explanation by impact and recoil, which received from the kinetic theory of gases an appearance of well-nigh universal truth and so long dominated the scientific imagination, now admits a rival if not a conqueror in the electrical theory of matter. We are no longer invited to view the world as a vast, intricate pattern of billiard-balls, but rather as a collection of charged corpuscles, streaming, oscillating, or grouped in systems. And the uniform time and space which Kant so confidently assumed may have to be given up, at the challenge of the doctrine of relativity. It is also well to remember that Darwin's theory of the origin of species has never met the demands of the palæontologists, who feel compelled to postulate an orthogenesis which the biologist has not been able to explain. In fact, the argument for the evolution of species, however strong it may be, proceeds by circumstantial evidence[1] and thereby lacks the demonstrative force of an experiment in physics, or a mathematical calculation. Yet these considerations have not prevented the pragmatist from building a philosophy upon the Darwinian scheme, or the mechanist from framing another upon the billiard-ball ideal, or either of them from assuming as a final truth that every event has a cause and that the physical energy of the universe remains constant. In particular, the latter assertion

[1] *Cf.* Morgan, *A Critique of the Theory of Evolution*, p. 9, p. 14 ff.

has been used by philosophers as a ground for denying the efficacy of mind, *viz.*, its initiation of change in the bodily processes of man. Some twenty years ago Münsterberg was wont to anathematize the interactionist as one who doubted that surest result of science, the conservation of energy. With an instant generalization from the sciences of the inorganic, and in the true Prussian spirit of system at any cost, he extended the application of this law to the whole visible universe, living and non-living alike. And the majority of philosophers and of psychologists—so far as the latter are willing to study the interesting problem of the relation between mind and body—have followed his example. Few are willing to lend an ear to the suggestions of animism or vitalism; few take the trouble to inquire how far the doctrine of conservation has been shown to hold of life or mind. Nor is the fault all on one side. If Münsterberg and the mechanists, imbued with fear of being considered unscientific, gave to a scientific law more than it ever claimed, a vitalist like M. Bergson roundly declares that that law has never been proved, and does *not* hold, of life and mind.[2] The French professor, whose statement was published in English in 1920, took no notice of careful experiments on the subject printed in 1903, which we shall presently consider and which seem to contradict his assertion. Even McDougall, who mentions[3] a brief report of these experiments without examination of their argument, summarily insists that the law can never be proved to hold of processes so delicate and complex as those of the nervous system: "The few experiments which go to show that the energy given out by an organism is equal in amount to the energy taken in, are far too few and too rough to rule out the possibility that psychical effort may involve increment of energy to the organism; for increments far too small to be detected might effect very important changes in the course of the organic processes."[4] The original record of the experiments in question Professor McDougall does not mention, and without analysis of them to see just what they do prove such general denials and affirmations are of little value.

The law of conservation of energy, so far as proved, is an empirical induction. In inorganic nature this indication seems to leave no room for reasonable doubt. The energy of closed systems of many sorts has been measured, with the utmost minuteness, at the beginning and end of some process and has been found constant within the limits of error in observation. It is no *a priori* matter; the measurements might have shown increase or diminu-

[2] *Mind-Energy*, pp. 43–45.

[3] *Body and Mind*, p. 93.

[4] *Op. cit.*, p. 220.

tion. Now life is *prima facie* different from the inorganic. It may not really be different in kind, but it looks so; and this renders the extension of the law problematic. However much a philosopher or biologist may attempt with the broom of the inorganic law to sweep the universe clean of mental energy and of other animistic "superstitions," the simple fact is that we do not know, without experiment, whether or not mind contributes energy to the organism. And it is of some interest to find out.

To be sure, there are systematists who believe the issue to have been exploded—pragmatists, idealists, behaviorists, *etc.* Differing on many points, they agree that the dualism of mind and body is an opposition of unreal abstractions, and therefore there is no question of the relation between them. It would be like asking whether the color of an orange affects its shape. Perhaps the illustration is an unhappy one, for just that can be asked and answered. It has been proved that light is a measurable energy. But it is easy to see that no such epistemological analysis of experience in general can settle problems of real interest; all that they do is to recast our phraseology. We are now to ask whether the law that holds of the abstractions called inorganic processes holds also of other abstractions called living processes, or between these two abstractions, *etc.* Again, the question might be stated thus: does the coordination of responses in living things have a measurable effect on the particular responses, and conversely? Or it might be put in quite materialistic fashion: does the energy taken into the living organism of man remain constant in amount during redistribution in the organism and return to the outer world?

Upon the question of fact we find a series of experiments of which most philosophers and even psychologists seem unaware. McDougall, as was said above, refers to these, but only to a very brief statement of their result in the Reports of the British Association for 1904. He has not given his readers an opportunity to estimate the validity of the arguments employed; and in virtue of the extraordinary care of the experiments and the crucial issue which they discuss, this is unfortunate. Accordingly it seems a plain duty to give some account of the investigation, in order that we may see just where the latest scientific evidence leaves us in the whole question. The experiments were performed by Drs. W. O. Atwater and F. G. Benedict and are recorded as Bulletin No. 136, U. S. Department of Agriculture, Office of Experiment Stations, under the title *Experiments on the Metabolism of Matter and Energy in the Human Body* (Washington, 1903). It may be that there are later experi-

ments; the author speaks of their desirability indeed; but I find no record of them in later Bulletins or elsewhere.

In an earlier research, conducted by Atwater and Rosa (same series, Bulletin 63, 1899, p. 11) the experimenters had said: "The views of specialists as to whether the law of the conservation of energy actually applies in the living organism are somewhat conflicting. So far as the writers can judge, the larger number of chemists, physicists, and physiologists who have at all carefully considered the subject assume that the law does obtain; basing that supposition on the *a priori* ground that there is every reason to believe that it must hold in the organic world, as it has already been demonstrated to hold in the inorganic world. Not a few regard the experiments already made, notably those of Rubner just referred to, as implying very strongly, even if they do not strictly demonstrate, the application of the law in the animal body. Others, however, question this demonstration, and there are some physiologists who, knowing from long experience the difficulties inherent in this kind of experimenting, the many sources of uncertainty and error, and the great amount of labor which is needed for reliable results, frankly avow their belief in the impracticability of any satisfactory proof that the law of the conservation of energy holds in the living organism."

In another Bulletin of this series (No. 45) a summary of previous experiments by other scientists is given; but we confine ourselves to Bulletin 136, which is by far the most thorough work done on the problem. The object of the labors here recorded was not simply to seek confirmation of the law of energy, but also to investigate the nutritive value of certain foods; and pp. 1–193 are mainly concerned with the bearing of the results of the latter question, while pp. 193–357 take up more directly the question of conservation. Though many experiments had been performed already on that subject, the authors thus testify to the need of further work: "The investigations of Rubner in Germany and of Laulanie in France had brought results fully in accordance with the law of the conservation of energy, but their experiments were made with small animals, dogs and rabbits, and were comparatively few in number; the experimental periods were rather short; the analyses of food, drink, and excreta were not carried out in great detail, and no experiments were made in which external muscular work was involved" (p. 193).

The experiments before us were made upon five men separately, placed in a specially constructed chamber, 7.5 by 4 by 6.5 feet, which was so designed as to determine the income and outgo of air, heat, moisture, *etc.* to and from the subject. "The total number of ex-

periments with measurements of income and outgo of energy in the body is 51, and the time covered by them is 150 days" (p. 99). They extended from May, 1897, to May, 1902. The chamber was called the "respiration calorimeter" because it determined the income and outgo of respiration, perspiration, and their products, and registered the heat given off from the subject's body. In each experiment the subject lived in the chamber, eating and sleeping there for a few days. The series was "divided into two classes: (1) Those in which the subjects were practically at rest, *i.e.*, had no more exercise than was involved in dressing and undressing, and care of furniture, food, and excreta; and (2) those in which they were engaged in more or less severe muscular work" (p. 99). "Of the 26 rest experiments, covering 72 days, several were with special diets, and four, covering a total of 5 days, were with the subject J. C. W. fasting" (*ibid.*). "The 25 work experiments, covering a total of 78 days, were all made with special diets . . ." (*ibid.*). "The larger number of rest experiments were made with E. O. [subject], and J. C. W. was the subject of the larger number of work experiments" (*ibid.*). The latter was a college athlete (cyclist) in prime condition; the work consisted in riding a stationary bicycle connected with an ergometer. All subjects were in excellent health during the experiments. The authors suggest indeed that "important results could be obtained also in studies of nutrition in disease . . . and other conditions more or less abnormal" (p. 10), but no results were sought under such conditions.

In order to test the conservation of energy, the income of energy in each experiment must be balanced against the outgo of substance, heat and motion. The income depends on the potential energy of the food and drink. Samples of these were analyzed and from the amount of them consumed and their equivalent in calories the energy of food and drink was determined. Subtracting from this the energy of the material which passed out in faeces and urine, we have the available store of potential energy laid up in the body. Not all of this is used, however—*i.e.*, oxidized and turned into kinetic energy (heat and movement) within the body; we wish to know just how much is used, in order to see if the amount can be equated with the energy given off from the body in the shape of heat or mechanical work. This energy of substance actually oxidized the authors' term "energy of net income." It is "represented by the available energy of the nutrients of the food (*i.e.*, potential energy of total food less that of the urine and faeces) minus the potential energy of the material gained" during the experiment. That is to say, if the subject gained so much of protein and fat, the energy residing in these

is stored in the body and takes no part in the transformation of energy which the experiment is studying. The gain of the body in protein and fat is estimated from the amounts of nitrogen and carbon given off in excreta, together with the increase in weight of the subject. If on the other hand the subject loses in body-material, the bodily reserve in protein and fat is drawn upon for energy of oxidation, and the amount lost must not be subtracted from, but added to, the potential energy of the food-substance retained in the organism. Thus we obtain the "energy of the material actually oxidized in the body" which is to constitute one side of the equation. The other side, the outgo, "consists of the heat given off and the external muscular work done" (p. 195). In the rest-experiments, the slight external muscular work "would naturally be converted into heat, as, for instance, in the impact of the foot upon the floor in stepping. . . . Roughly speaking, we may say that all the potential energy made kinetic in the body by the oxidation of food and body material left the body as heat, and that this made the net outgo of energy" (p. 194). And we must add that "so delicate were the measurements of temperature that . . . if the man inside [the calorimeter] rises to move about, the increase in the heat given off from his body with this muscular work shows itself in a rise of temperature which may be immediately detected" (pp. 11–12).

"In the work experiments a certain amount of energy is given off as external muscular work, and this added to the heat given off from the body makes the net outgo" (p. 194). It must also be mentioned that the quality and composition of air inhaled and exhaled, as well as of perspiration, were taken into account. In fact, as one peruses the report, it appears that few if any sources of error which ingenuity could suggest were unconsidered. We have given as much of the method as seems necessary, and may let this suffice for description of the conduct of the investigation. We now turn to the results.

"If the law of the conservation of energy obtains in the living organism, the net income and the net outgo of energy should be the same. In such physiological experimenting, however, it would be hardly fair to expect the figures for the two to agree for each day of a given experiment or for each experiment as a whole. . . . There may be errors in the estimates of the amounts and heats of combustion of the materials actually oxidized. Variations due to irregularities of the physiological processes of the body are unavoidable, and may materially affect the results. But . . . these errors would tend to counterbalance one another in a series of experiments, and . . . in the average of a sufficiently large number of

experiments . . . the income and outgo would be very nearly the same'' (pp. 196–197).

'' Exactly this is the case in the data here reported. The variations for individual days, and even those for the individual experiments . . . are not inconsiderable, but considering the average of all the experiments the agreement is very close'' (p. 197). In the 25 days of 7 rest-experiments we find extreme variations ranging from —6.5 to + 9.1 per cent. of income in the energy given off. In the average of 14 experiments, however, as given on p. 123 (in Table 79) we find that the subject gave out per day 5135 calories, and oxidized within his body 5143 calories—a difference of less than one fifth of one per cent.

Now the question is, how do these experiments (the most careful, we may fairly say, hitherto conducted) show that the law of conservation holds throughout the field of organic process? Let us see if anything was taken for granted in the method which from an empirical point of view needs proof. First, it was assumed that the heat of combustion of a substance, already ascertained by combustion *outside* the body, will be the same when combustion occurs *within* the body. But surely, one says, this is permissible, else we can make no calculations of energy here. We grant it; we wish merely to point out that this *sort* of assumption —of a sameness of process between inorganic and organic—must not be carried beyond necessity. Just that, however, seems to be done in the second assumption we notice, *viz.*, that the potential energy actually oxidized in the body is the potential energy of the food taken in and kept, minus the stored tissue (or plus lost tissue). The assumption is equivalent to saying that the more of a given income is laid up as tissue, the less remains to be burned, and the more is burned, the less remains as potential energy of tissue. This implies that the energy of a given amount of food-substance taken in remains a definite and constant sum during its redistribution in the organism. How do we know this? Is it not assuming that the conservation of energy holds in the body? In short, are we not basing our measurement of the energy due to oxidation which is to give one side of the desired equation on the supposition that what we want to prove is true? It may be urged that there is no other way of estimating the ''net energy of income.'' If that is so, it only means that there is no way of demonstrating the conservation of energy in the living body.

As far as the observations of fact go, we do not know but that some of the energy taken in and kept as food, and not stored as protein or fat, is not oxidized at all, but goes into some form of energy of a non-physical sort—mental energy perhaps, or what

you will. It might even be lost, or increased in amount. These suggestions may be dismissed as absurd; but all we here urge is that the present experiments make no decision about them. Their validity must be settled on other grounds than the results presented by Drs. Atwater and Benedict. Or we might suppose that some of the energy stored as protein and fat disappeared into an unknown form, or was even lost. There is no direct measurement of this stored energy; we do not know by plain observation whether it remains constant until drawn upon for oxidation. In inorganic nature, to be sure, it would be quite groundless to allege any change of amount without some knowable physical reason. But the conservation of energy has been proved of inorganic nature, and if we could argue from that to organic, there would have been no demand for experiments on animals.

There is however no need of straining our credulity on the matter. The easy alternative is to suppose that some of the income of energy is transmuted through the agency of the nervous system into certain mental states, such as sensation, perception and other recipient psychoses, and that some of this is translated back into bodily movements through the conative and active psychoses. This is the familiar hypothesis of interaction, and it is quite consistent with the experiments before us. We may not be able to define or measure the energy of mental states, but that does not make transformation impossible. And it might even be the case that on the whole as much energy went into the recipient states—sensation, *etc.*—as came out in the active states; so that the conservation of energy would still hold true. But whatever supposition we make on that head we do seem to find that the results of these very conscientious experiments do not render unlikely the transfer of energy from body to mind or vice versa; and the fulminations of Münsterberg and other "parallelists" appear to have no just warrant from science. And the transfer need not be of a very minute amount, as some interactionists have felt that it must be. It is not that the conservation is *nearly* proved; it is not proved at all—having been assumed in the measurements. Nor shall we be compelled to imagine some device by which mind may intervene and switch off a nerve-current or release a potency, without doing physical work. The experiments do not rule out mind from doing or suffering a very considerable amount of work.

But while we conclude that the conservation of energy has not been proved, or even approximately proved, of the living body, we believe the experiments have a very great and positive philosophic value. This may be brought out by an objection to our conclusion. For it might be urged that even though conservation is not proved

throughout the process of redistribution in the body, still it *is* proved that the quantum of energy that comes into the body and is retained, minus what is known to be stored, is equal to the quantum that comes out. This equality between beginning and end of the whole process is striking. If some of the energy given out comes *from*, say, the mind, and some of the energy taken in goes *to* that same, why is it that there is always found at the end just the amount present at the beginning? There must be some explanation of this equality, and surely the law of conservation, without recourse to a mysterious mental energy, is the natural one. Without that law, equality would hardly be so invariable. Unless, then, we can find some other reason to account for it, conservation would still be in order. To this objection we answer that such a reason can be found in the shape of a certain general law or tendency holding between organic and inorganic, as well as between various forms of life. This law can be shown on independent grounds, and has in one form or another long been recognized. It is the merit of the experiments before us to confirm it by measurements.

It is very probable that living beings would develop the habit of giving out a quantum of energy as great as that which they take in. It is, in fact, a necessity that they do so; else the store of energy in the environment, upon which they draw for subsistence, would gradually be depleted, and life must perish. Energy passes through a cycle, going from the outer world into living matter and out again; and the balance must be kept even. The energy which passes out from animals in the form of excreta goes into the soil and atmosphere, and serves to sustain the plants; the energy which the plants store in their tissues serves as the food of animals. This energy, if it diminished in amount in its passage through the cycle, would become less and less in the course of years, until finally it would not suffice for the maintenance of life. The kinetic energy given off by an animal body—heat and movement—which does not directly assume a form available for animal food, is conserved, as the law of conservation holds for all inorganic processes; and it is used by animal life when the latter obtains its food. The animal profits by energy of position, of meteorological processes, of elasticity, and other natural forces, in order to get his food; and fortunate it is for him that these are maintained at a relatively constant amount. To be sure, the amount is only relatively constant, for the heat-energy is being radiated away and lost. All the greater is the need for his returning undiminished the stock of energy which has passed through his own organism. The same reasoning holds, *mutatis mutandis*, of plant life and the bacteria. All living things must maintain the level of energy

in the material world; they would otherwise not survive. And when we consider the enormous numbers of living things, from the millions upon millions of bacteria through the nearly ubiquitous green plants and the ten phyla of animals up to man, we see how even a slight loss of energy by each individual would be magnified and would check the vital sustenance. Life itself, we infer, must early have become the kind of process that pays back what it borrows, almost to the last farthing; the least habitual failure to pays its debts, multiplied by the uncountable mass of life, would ere long be fatal. As in commerce so in life the balance must be kept. It would not matter whether the conservation of energy held in the passage through organisms; the physical energy taken in might be partly lost or transmuted into some psychical mode. Nevertheless the living being would, on pain of extinction, have to see that an equal amount of energy was restored, whether from the body or from some other reservoir or *ex nihilo*. Putting the thing in non-teleological terms, we may say that a kind of life which did not give out at least as much energy as it took in would become extinct. Natural selection, if we may use the phrase here, would weed it out.

It might be thought that though this would be true on the whole, yet individual cases might show a falling away from the law—as they do in the commercial world. But remembering that the tendency to repay, in order to be so general, must be deeply implanted in the nature of life, we find less reason for variations. Nevertheless we might well expect, in particular cases, some deviation; though it would diminish as we took averages. This is, in fact, just what we do find. The experiments showed individual departures from equality of income and outgo as great as —6.5 or + 9.1 per cent., and we can not say that this is wholly due to errors in estimation.

But why should organisms not give out *more* energy than they take in (minus storage)? If they did so, the stock of available energy in the environment would be increased, and on the whole living matter would thereby profit. The animal and plant kingdoms would give more and more sustenance to each other; heat-energy, which is favorable to life, and other forms of energy which are used by organisms in food-gathering, would grow greater, and as a result life would thrive and multiply. But by that very fact the income of energy to organisms would be increased; for vigorous organisms take more sustenance. The balance between the two poles, life and environment, would continually tend to become even. And sooner or later a limit of increase must be reached. The available space and matter would in the end put up a bar; but long before these were exhausted other conditions would interfere. The physical properties of C, O, H, and N would not permit them to receive and pass on more

than a certain amount of energy without injury to the organism. If life were the sort of process which resulted in excess of outgo over income, it would almost certainly in the long course of its history have reached that limit, and then the balance would be speedily restored—else life would be destroyed. By this time, then, life must have acquired the habit of equilibrium between income and outgo.

Nevertheless the conditions are not quite the same as regards excess and deficit of outgo. Both tend to disappear, but excess would do so more slowly than deficit. The former does not, like the latter, work toward a decrease of food-supply; natural selection will not so soon prevent it. Restriction of excess of outgo will thus probably be a later acquired habit of life—less deep-seated, more subject to variation. And if variations from the habit occur, they are more likely to occur, evidently, in creatures possessing a highly developed nervous system. For two reasons, then, we may expect that occasionally an organism like that of man would display more energy than its income from the external world would amount to, namely (1) because the habit of giving out *no* more energy than it takes in, being more tardily acquired, would command less implicit obedience than the contrasted habit and (2) because deviations from the rule are more to be expected in highly developed and complicated nervous systems than in simpler ones. Even so, however, the exceptional output of energy would hardly take place in such automatic processes as nutrition, excretion and the like. These, which are controlled from the cerebellum, are as a rule not accompanied by consciousness. This attribute of organisms is called into play when a conflict of impulses inhibits the customary reaction of stimulus; a novelty, a response which departs from the habitual, is the occasion of it. The special sort of nervous process which goes with consciousness is therefore the most likely to be the scene of a departure from the usual balance of income and outgo. We should actually look for increased output of energy over income in cases when an idea, an ideal, a conscious effort against opposing motives or bodily inertia, seems to govern the behavior of the organism. In such instances there might well be displayed in the body and as outgo from it, an amount of energy which would very considerably exceed that of intaken food. We do not now assert that this happens; only that it is possible or likely, and that the scientific measurements of energy in the human body do nothing to remove the possibility. Such examples as James detailed in his well-known theory that men may occasionally tap higher reservoirs of energy than those customarily used, would fit the hypothesis. The many cases of cure of disease by faith, prayer, suggestion, or other mental processes would illustrate the same. What prevents the

sophisticated thinker of today from taking these cases at their face-value is the fear of being thought unscientific; whereas, if our argument is correct, science has nothing to say against them.

But in any case, interaction between conscious process and bodily process would seem to be more credible than a parallelism with its closed circle of physical energy. While of course the normal equality between income and outgo might be due to the conservation of that energy alone, it might also be due to the action of natural selection, destroying in the end a kind of life that did not by interaction keep the balance even. The facts established by the experiments before us give no ground for preferring the one explanation to the other; and accordingly the view natural to experience and common sense, the view of interaction, seems the more reasonable. The philosopher and psychologist, overawed by what they hastily assumed to be a dictum of science, seem to have renounced what science had not asked them to give up, and fled when no foe pursued.

Finally, we repeat that is not a question of finding some device by which mind may influence a nerve-current without doing work—as by a switch or a releasing of potential energy. We need not content ourselves with so feeble a prerogative; the experiments have shown no reason why mind may not do a great deal of physical work. Normally, to be sure, the amount of that work would be such as to keep the equilibrium between inflow and output of energy; but in exceptional cases mind might send forth an amount which would far exceed the volume taken in through the usual physical channels.

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## INSTINCT AND CAPACITY—II

### Homo Domesticus

MAN has been dignified by science with the title *Homo sapiens;* but his wisdom is the wisdom of his traditions. To the anatomist the cerebrum looms large; to the anthropologist—institutions.

History "records the transfer of power from one mystically sanctified source of authority to another, from a church to a book, from a book to a state, or to an intangible public opinion. But with unfailing tenacity every society from the simplest to the most complex has adhered to the principle that the one unpardonable sin consists in setting up one's private judgment against the recognized tribal authority, in perpetrating an infraction of tribal taboos."[1] If the name of the species were based on its behavior man would be called *Homo domesticus.*

1 Lowie, *Primitive Society,* p. 440.